Porto Alegre, 09 november 2016.

Title: Sarcopenia and non-alcoholic fatty liver disease: is there a relationship? A systematic review

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Dear Editor of WJH

We thank you very much for the comments from the reviewers. All of them are of great importance and interest to us, and we tried to address them by answering or justifying all questions.

The modifications are highlighted in the text in yellow. We hope we have met the reviewers' expectations in regards to the questions, and also that you appreciate to read the manuscript again.

Reviewer 03402733

Cristiane V. Tovo et al. et al. performed a systematic review, in which authors focused on the relationship among NAFLD and sarcopenia. This review manuscript encompasses an interesting topic and may be worthwhile publishing, however, the number of the study included in the analysis was only three. The methods were suitable for a systematic review. Major comments:

1. Three studies included in the analysis were cross-sectional studies. Thus, authors couldn't claim a causal-relationships. Then, authors should not say a risk of NAFLD. Instead of it, authors could claim that there were an independent association between sarcopenia and NAFLD.
   Answer: This is true! This was changed along the text (lines 69-71; 81-83; 261-262; 342)

Reviewer 03304651

Dear author, Greeting from me! The clinical characteristics of Sarcopenia is low skeletal muscle mass and increase in body fat; currently there are several studies showed there are association between sarcopenia and non-alcoholic fatty liver disease.But lack of large sample study. This manuscript belonged to a systematic review,however, it has the following problems:

1. Page 3, line 71, the punctuations between keywords should be comma .
   Answer: This was modified in lines 72-73.

2. Page 10, line 193, the Meaning, which ” The searches were performed with and without limiting the types of articles (RCTs, clinical trial, comparative study).” is not clear.
   Answer: This is a mistake, and is now corrected in line 193.

3. Forms are not standard, and the references between result (15.16,20) and Table 1 (18,19,14) have diffenent,why?
   Answer: This was corrected in the Table 1 (line 466).

4. Some grammatical and syntax errors in this manuscript need to be corrected.
   Answer: All the text was reviewd about this aspect and many modifications were done.

Reviewer

This review is timely as there is emerging evidence and understanding of the association between NAFLD and sarcopenia. However, there remains much unanswered gaps in our knowledge with regards to this relationship.

There are a few issues to address:

1. The statement in the introduction about 3% prevalence of NASH among obese non diabetic subjects needs to be revised. It seems a bit inconsistent with current literature. On review of the reference, it appears that 912 obese

patients were enrolled but only 33 patients with altered liver enzymes had liver biopsies done, of which 29/33 (88%) had features consistent with NASH.

Answer: This could be an interpretation of the data, despite the authors of this study published in 2002 prefered to use the first. Attempting to the reviewer, we changed in the text (line 127, page 7).

2. The search selection of the eligible articles should be updated since 2015; I have found a few more relevant articles such as Koo BK et al J Hep 2016 and Kim et al Hepatobiliary Pancreat Dis Int. 2016 Feb;15(1):39-47.

   Answer: This are very recent publications and were not published yet at the time of the systematic review was done. Now they have been incorporated to the manuscript. However, they evaluated different primary outcomes of interest, and are not included in the systematic review, but cited only in the discussion.

3. The discussion should be expanded in regards to postulate the possible pathways linking liver-muscle adipose tissue axis; ie role of myostatin/adiponectin in IR/muscle mass control/inflammation and what is the clinical significance/relevance of this relationship in NAFLD.

   Answer: This was already addressed in the text in the introduction (lines 140-148). But it should be considered to be included in the discussion if the reviewer consider it would be a better strategy.

4. It is interesting to note that all three studies reviewed are Korean studies, so there may be limitation to extrapolate these data to other populations; Asian BMI thresholds for obesity are much lower than Caucasian, body compositions, including muscle mass are also diferent.

   Answer: This is an interesting observation and was already addressed in the limitations section (line 326-331).